in the future, of course, and Tomonaga's motivation was to provide a sounder basis for the then unsatisfactory quantum theory of fields but without any specific applications in mind.

Schwinger's entirely independent development of a covariant formulation was remarkably similar in spirit to Tomonaga's, and as remarkably different in spirit and in detail from Feynman's slightly later work. It was Schwinger who first saw the problem whole, who first grasped the relationship between the Lamb shift and the anomalous magnetic moment and who first calculated both. About this work Schweber says:

> The importance of Schwinger's calculation cannot [should not] be underestimated. In the course of theoretical developments there sometimes occur important calculations that alter the way the community thinks about particular approaches. Schwinger's calculation is one such instance. By indicating, as Feynman had noted, that "the discrepancy in the hyperfine structure of the hydrogen atom . . . could be explained on the same basis as that of electromagnetic self-energy, as can the line shift of Lamb," Schwinger had transformed the perception of quantum electrodynamics. He had made it into a effective, coherent, and consistent computational scheme to order e^2.

Feynman's approach was entirely different and absolutely extraordinary. So innovative and so breathtakingly original was his formulation and so incomplete and sketchy was its foundation, that the first reaction to it was skeptical and even negative. It was in some measure a set of rules for calculation, using the now famous and ubiquitous Feynman diagrams, with positrons depicted as electrons moving backward in time. But it soon became apparent that the rules worked and were incomparably easier to use in doing calculations than was Schwinger's formulation. And soon, too, the foundation was filled in. Schwinger remarked, not exactly admiringly, that Feynman had brought QED to the masses.

It was Dyson who completed the development of QED in two ways: first, by showing that Feynman's and Schwinger's formulations were equivalent and, second, by showing that renormalization worked to all orders, that there were no infinities remaining after mass and charge were renormalized. Both were major accomplishments. Schwinger's approach, as was Tomonaga's, was that of a field theorist; quantum fields were the primary constructs. Feynman's approach, at least initially, eliminated all references to fields and focused instead on particles (electrons and photons) and on their space-time trajectories. Neither understood the other, and their only point of contact was that each approach yielded the same answers. Dyson's demonstration of their equivalence was a great triumph. Even more so was his second accomplishment, which required the most penetrating kind of analysis. "His perception and power," Frank Yang wrote, "were dazzling."

Schweber argues that Dyson should have shared in the Nobel Prize awarded to Tomonaga, Schwinger, and Feynman in 1965. Many, this reviewer among them, do not agree. The difference between clarification and innovation is all the difference.

One of the pleasures of *QED* comes in reading what Schweber calls "loving biographies of the principals involved and an admiring account of the community of theoretical physicists." Loving and admiring, yes, but the airbrush has been sparingly used, the warts (most of them) show, and so do some sharp tongues. Much is presented in the principals' own words, and that is all to the good, for they are livelier and more graceful than Schweber's. Some wonderful anecdotes and vignettes are presented; the reader is referred to the interview with Dirac on pages 18–20 (excerpted on page 1889 of this issue of *Science*) for a delightful example.

Assessments of the character of the principals and of the relative importance of their contributions involve, of course, matters of judgment about which reasonable people can (and will) disagree. But all will agree that Schweber has presented the story fully and fairly enough to enable readers to draw their own conclusions.

Schweber is neither psychologist nor sociologist, and his efforts in such directions happily are limited in number and extent, although the fact that the cast of characters is almost exclusively white and male ought at least to have been noted. Schweber is no philosopher, either, but then neither are the principals. Schwinger was indifferent to philosophical discussions of science, Feynman disdainful. When Dirac was once asked to express his philosophy of physics, he wrote a single sentence: "Physical laws should have mathematical beauty."

The point to be made here is that the philosophy of physics is not physics. The same, of course, is true of the history of physics. As a result, working physicists tend not to give serious attention to either, even when they should. No matter. It is the scientifically literate reader for whom this book is intended, and not merely the working physicist. It is not an easy read but is well worth the effort and, once started, hard to put down.

QED is important, worth reading about, and worth writing about, because it accounts—with extraordinary precision—for the properties and interactions of those most common, accessible, understandable, and fundamental of objects, the electron and the photon. But what is more, QED sparked a revolution by providing a model for the application of quantum field theory in other domains. In particular, the renormalization concept has been remarkably fruitful in such fields as condensed matter physics. Further, the view that renormalizability is not a problem but a basic requirement led Steven Weinberg and Abdus Salam to their great unification of electromagnetic and weak interactions, electroweak theory. The concepts embodied in QED have also been extended to the strong interactions, where the analogous developments are denoted by QCD (for quantum chromodyamics).

Perhaps the ultimate irony is that the men who made QED took a rather limited view of its validity. Feynman had sought a divergence-free QED but concluded that he had merely swept the infinities under the carpet, as he put it in his Nobel acceptance speech. Dyson had hoped to prove that the renormalized QED perturbation expansion converged, but proved the contrary to his great disappointment. And Schwinger devoted his later years to the successful construction of a divergence-free alternative approach (source theory). Not surprisingly, however, the next generation found the ideas of renormalizable QED far easier to accept uncritically than did the founders.

A final word. Among his many other accomplishments, Schweber does a very good deed in giving to Dirac the credit he deserves. Schweber emphasizes that Dirac towers above everyone else in his influence and that, along with many others, the four who made QED were all "students" of Dirac. Perhaps, with justice, the book could have been titled "QED and the Men Who Made It: Dirac, Dyson, Feynman, Schwinger, and Tomonaga." (The order remains alphabetical, please note.)

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Experimentalist's Career

Antoine Lavoisier. Science, Administration, and Revolution. ARTHUR DONOVAN. Blackwell, Cambridge, MA, 1994. xvi, 351 pp., illus. $29.95 or £35. Blackwell Science Biographies.

Although widely known as a founder of the Chemical Revolution who was guillotined in 1794 during the Terror, Antoine Lavoisier devoted only a small part of his public career to science. As indicated by its title, this new biography attempts to broaden our view by portraying not only the chemist but also the "other" Lavoisier—the barrister, tax official, agricultural reformer, financier, director of the Gunpowder Ad-

Antoine Lavoisier while in prison. [From the dust jacket of *Antoine Lavoisier*]

ministration, spokesman for the Academy of Sciences and its many committees, and sometime (surprisingly liberal) political theorist. Yet amidst Lavoisier's varied pursuits Arthur Donovan finds a career unified by two themes: an "18th-century version of positivism" based on reasoning by experiment, and a driving, almost ruthless ambition. Above all, concludes Donovan, Lavoisier "played to win."

Like the other popular biographies in this Blackwell series edited by David Knight (to date, works on Galileo, Newton, Henry More, Humphry Davy, and Darwin have appeared), Donovan's book blazes few new trails but rather provides an elegantly written synopsis of the existing scholarship on Lavoisier and on 18th-century France and its sciences. This is a biography overflowing with context. In rich detail we learn about the Order of Barristers, French royal finances and tax collection, the French government's campaign against Mesmerism, the manufacture of gunpowder, and of course about the political crises that culminated finally in the demise of the Old Regime. In more abbreviated fashion we learn about the practices and concepts of 18th-century chemistry that Lavoisier sought to transform.

Donovan's Lavoisier played his many public roles with the gestures of an 18th-century experimental physicist. From his teachers Jean Antoine Nollet and Nicolas Louis de Lacaille (both also renowned instrument-makers), Lavoisier learned that certain knowledge could be attained only via experimental reasoning based on precise instruments, analytic quantification, and restrained generalization. Hence did Lavoisier make the balance, the calorimeter, and the eudiometer the central fact-producing machines in his chemistry. He bragged to Benjamin Franklin that his *Elementary Treatise on Chemistry* (1789), by following the "torch of observation and experience," would "make chemistry appear quite like experimental physics." Likewise, Lavoisier tried to rationalize accounting procedures in the Company of General Farmers, the powerful tax-collecting group he joined in 1768. Later, as a member of the newly created provincial assembly in Orléans, Lavoisier sought to base his many proposals regarding refinancing public debt, welfare, wool production, or navigation on quantitative facts rather than political arguments. Just as in his chemistry Lavoisier tried to convert questions about theory into questions about experiments, so in his public life did he try to convert political issues into matters purely administrative.

If Lavoisier learned his method from Nollet and Lacaille, his ambition apparently came ready-made. Although this biography tells us little about the private Lavoisier, his personal relationships, or his self-understandings, Donovan's public Lavoisier was ruthlessly efficient. Whether it was his campaign for admission to the Academy, his marriage to the daughter of a senior partner in the Company of General Farmers, his astonishingly successful reform of national gunpowder production, or his well-known crusade for a new language of chemistry, Lavoisier invariably showed a sophisticated knowledge of available cultural resources that he then skillfully deployed to realize his goals. Only in the end did he misunderstand how profoundly French political culture had changed. Indeed, one of the puzzles left unresolved by this biography is whether Lavoisier simply lacked the political acumen to survive in the treacherous times of the Terror or chose martyrdom in allegiance to certain political principles.

"A cartoon of Benjamin Franklin brandishing the report of the royal commission on mesmerism and the mesmerists fleeing a disrupted seance." [From *Antoine Lavoisier*]

Donovan's portrait of the "other" Lavoisier powerfully melds the histories of a public figure, a nation, and its science. Of the half-dozen major biographies of Lavoisier written over the past half century, this one surely offers the most comprehensive and accessible account of the self-acclaimed founder of the Chemical Revolution.

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Other Books of Interest

Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a. Scientific Correspondence with Bohr, Einstein, Heisenberg, a.o. WOLFGANG PAULI. Vol. 3, 1940–1949. Karl von Meyenn, Ed. Springer-Verlag, Berlin, 1993. lxiv, 1070 pp., illus. DM 216 or ÖS 1,684.80 or SFr 212. Sources in the History of Mathematics and Physical Sciences, vol. 11.

This third volume of Wolfgang Pauli's scientific correspondence contains as many letters—some 500—as the first two volumes together. It covers the years Pauli spent in the safe haven of the Institute for Advanced Study in Princeton during the Second World War and the first few years following his return to the Eidgenössische Technische Hochschule in Zurich in 1946. It is the first volume for which von Meyenn gets full editorial credit, though he has effectively carried the full editorial burden from the inception of this project some 20 years ago. It is, also, the first volume in which a significant fraction (roughly half) of the letters are in English. Indeed, it is interesting to see this language not merely being used by Pauli himself with his American correspondents—this he had already begun to do in the mid-1930s—but also displacing German in his correspondence after the war with Bohr and with nearly every other Danish, Swedish, and Dutch correspondent. Pauli's wartime America is not that familiar to historians of physics: it is a lonely world, the world left behind as the Americans, and all but the most recent emigrés, were drawn off to war research.